The British Journal for the History of Science (2026), **59**, 115–119
doi:10.1017/S0007087425101416

ESSAY REVIEW

How to create a discipline? Looking back at luminaries

K. Brad Wray (ed.), *Kuhn's* The Structure of Scientific Revolutions *at 60*

Cambridge: Cambridge University Press, 2024. Pp. 326. ISBN 978-1-009-10070-0. £85.00 (hardback)

Arnold Thckray, *Science: Has Its Present Past a Future? Selected Essays of Arnold Thackray* (ed. Jeffrey L. Sturchio and Bruce V. Lewenstein)

Ithaca, NY: Seavoss Associates Publishing, 2022. Pp. 667. ISBN 979-8-5511-6773-0. £19.00 (paperback).

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Thomas Kuhn's *The Structure of Scientific Revolutions* (*SSR*) is one of the most widely read and cited academic books. However, historians of science have widely rejected it as simplistic, naive or even 'leftover Hegelianism' (see Kindi in Wray, p. 257). Although philosophers, historians and other scholars generally find ideas that are now over sixty years old somewhat old-fashioned, there remain, perhaps unexpectedly, merits to those ideas. In what follows, I evaluate these two books by pointing out how historiography of science as a discipline fits Kuhn's ideas about science itself, by analysing a correspondence evident in the *Selected Essays of Arnold Thackray*.

According to Kuhn, history of science is characterized by periods of normal science and revolutions. During the period of normal science, scientists in a particular field share a paradigm that includes, among other things, *exemplars* of successful solutions to established puzzles and the *promise* that the way the puzzles were solved can be reproduced. However, normal science produces a stockpile of problems it cannot solve (anomalies). Over time, these anomalies generate a crisis, followed by a scientific revolution. After a revolution, a new paradigm is adopted. Knowledge does not accumulate in any traditional sense, as different paradigms differ fundamentally in how they view the world. This is a summary of Kuhn's well-known views. But there are more nuances than meet the eye.

Chapters in K. Brad Wray's *Kuhn's* The Structure of Scientific Revolutions *at 60* cover topics from philosophy of language to scientific books to science education. While this heterogeneity is sometimes a bit unsettling, and some parts of the books may repel readers not deeply engaged in philosophy of science, they make certain aspects of Kuhn's thinking clear: Kuhn thought that science needs exemplars and that scientists need to be educated within the framework of the prevailing paradigm. After their education, scientists solve problems within the paradigm and especially in the light of the paradigm's exemplars.

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https://doi.org/10.1017/S0007087425101416 Published online by Cambridge University Press

Scientists become specialists who are separated from 'laymen' through their deep understanding of how problems can be solved and how their solutions can be communicated and evaluated. Although there might be external influences on what science attempts to achieve, the scientific community is a *closed* one. It is sealed off by the paradigm: either you know and adopt the paradigm, or you are not a scientist in that field.

These perspectives on science as a closed community and the relations between a paradigm and methods of communication, education and so forth are the most fruitful parts of the collection on Kuhn's thinking. Equally interesting is the discussion of how a paradigm becomes established based on a promise, not a track record. More important than past solved puzzles is a paradigm's promise to solve problems – a new paradigm simply cannot have a track record before it is accepted (see Haufe in Wray). For example, scientists saw in Newton's derivation of Kepler's laws not just a single solution but 'a whole strategy for setting up and solving problems of motion' (p. 179) – a methodological template for future research.

When one reviews the development of historiography of science as described by texts in the collection on Arnold Thackray's thinking, and looks *through* the texts, as the first ones were published some sixty years ago, a striking match between the development of historiography of science and how Kuhn describes scientific development becomes apparent.

First, one encounters Thackray's articles about the history of chemistry that discuss the topic in serious detail and explain faults in older accounts of that history. The way the book begins makes one expect a continuous flow of accounts of history of chemistry but instead the reader is quickly drawn into meta-level discussions on science and historiography of science. Next one encounters articles that discuss how science was part of society, what the relationships between societies and science are, and how men of science were integrated into their local societies to gain resources, recognition and status from other members of the societies, especially in Manchester and North America. As the book progresses, more and more attention is given to the development of historiography of science as a discipline and to the topic of science in the current world. Certain repetition is to be expected in a collection of essays that one author wrote for different audiences, but the reader may get a little frustrated with constantly facing statistics related to the growth of historiography of science as a discipline during Thackray's time, such as faculty positions, student enrolments, dissertations completed and so on, and constant discussions about the relevance of atomic weapons and related issues to the need to understand science that, while still relevant today, have lost their original motivating urgency.

While this is a shortcoming, it is also a clue for us. The time and effort Thackray has spent discussing the discipline and its identity tells us about a process of cementing a discipline: historiography of science. Thackray discusses its history, often in a critical light, but we also read about the (truly) remarkable effort George Sarton put in developing the field and its intellectual infrastructure. This is where Kuhn's picture of scientific disciplines comes into play, especially through the details that chapters in Wray's volume highlight.

First, one can view the detailed studies that open the collection of Thackray's work as setting exemplars for certain studies of history of science, especially related to chemistry. They show how certain details can be tracked and how science can be made local. Remember, setting such exemplars is, according to Kuhn, part of establishing a paradigm for doing certain types of research. Second, there is considerable discussion about the *promise* of how historiography of science may answer certain questions that require(d) answers, at least in the context in which Thackray wrote. The promise to answer certain questions is also, in Kuhn's model, a factor that makes a paradigm appealing, as Haufe's contribution to the Wray

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volume observes. Again, what the collection on Kuhn says is applicable to transformations that Thackray was creating through his texts and questions.

Third, these questions are 'external' insofar as their motivation stems from the role of science in the twentieth century. However, Thackray argues that historiography of science can address these questions through resources and approaches unique to the discipline that require specialized training to master. In Kuhn's model, science can be shaped, motivated and so on by external conditions, but how the questions are answered is an issue to be solved within a paradigm. Fourth, there is a great deal of discussion, largely philosophical, about the nature of historiography of science. This type of discussion comes to the fore during scientific revolutions, according to Kuhn's model.

Fifth, Thackray discusses at length and in many places the education of historians of science and their increasing professionalism. As Hasok Chang notes (in Wray), the education of scientists is a topic of importance for Kuhn's view on science. In order for there to be scientists who follow a paradigm, there must be a mechanism to educate the scientists. Sixth, Thackray also discusses the funding of historiography and, relatedly, the publication channels of historiography of science. These are especially clear in his personal reflections and in the history of George Sarton and the journal *Isis*. How science is communicated is also an issue Kuhn's discusses, as noted in Wray's collection.

While it *clearly was not* Thackray's intention to prove that historiography of science is a discipline that fits Kuhn's model of science, one can find most of the elements of Kuhn's account present in the historiography of science as presented in Thackray's collected essays. To offer a provocation, the collection reads almost as a proof that the historiography of science fits Kuhn's ideas and thus is a real discipline.

Let us return to the present day. Here we face perhaps the greatest irony. Kuhn noted that every discipline rewrites its own history for the purposes of creating a progressive identity. In the present day, historians of science pay little attention to Kuhn – or Sarton or many of the ideas Thackray presents as important in history of science (such as 'genius studies' (p. 274)). As noted, sometimes Kuhn's ideas are dismissed or seen as a burden (Kindi in Wray, p. 267), or the ideas have been taken as obvious truisms since the beginning (Wray, p. 2) and thus are not worth mentioning.

I find it implausible to think that Kuhn and thinkers like Thackray (I say 'thinkers', not 'only' historians, as Thackray's interest in science has been so wide) have not played a crucial role in shaping what scholars in historiography of science currently think about their subject matter, particularly through their discussions of the role of the past in understanding present questions (see the preface in Thackray). Some scholars may not have read *SSR*, but there is what I would call the issue of 'diffusion'. When a book has sold millions of copies and is omnipresent, the ideas in it flow back into the discipline from all directions (or, to use the notion of current historians of science, there is wide *circulation* of knowledge).[1] People discuss the work with those familiar with *SSR*, undergraduates hear about it even if they do not read it, the notion of paradigm has been used extensively, and so on. The ideas percolate, no matter how much of a burden they are – or even for that very reason: the work's incorrectness needs to be clarified (to the extent that this ritual has become tiresome).[2]

That current historians cannot engage seriously or admit the importance of ideas presented decades ago seems very much like, to use Kuhn's notion, *Orwellian history* (see Wray in Wray), where the history of a discipline is constantly rewritten by the discipline and

[1] See, for example, James A. Secord, 'Knowledge in transit', *Isis* (2004) 95(4), pp. 654–72; and Kapil Raj, *Relocating Modern Science Circulation and the Construction of Knowledge in South Asia and Europe, 1650–1900* (New York: Palgrave MacMillan, 2007).

[2] On this theme see Lorraine Daston and Sharon Marcus, 'Undead texts and the disciplines that love to hate them', *Public Culture* (2020) 32(2(91)), pp. 349–54.

https://doi.org/10.1017/S0007087425101416 Published online by Cambridge University Press

works of the past made invisible to serve the present-day identity of the discipline. I dare to guess that Kuhn's ideas, again, fit the historiography of science even with regard to this issue. There is a rewriting and reframing of the history of the discipline of historiography of science. This activity attempts to veil ideas that even today shape it to keep the identity of the discipline coherent and plausible.

Surely, we can notice that the historiography of science is a much wider and much more detailed discipline than imagined by those who wanted to provide an overall view of science (like Kuhn) or those who thought historiography of science was important, at least partly, because of the present-day issues that science created (like Thackray). Not so long ago, I reviewed *Debating Contemporary Approaches to the History of Science* in this journal and noticed two issues.[3] First, historiography of science is currently a pluralist discipline with several approaches. Second, even with some background knowledge in historiography of science, understanding any subfield – even a chapter that presents it in a simplified form – is difficult to a person without deep expertise in that subfield. This is a sign of the maturity of the historiography of science and, as I noted, shows that the historiography of science has become *historiography*.

This is exactly what one would expect after reading Kuhn. A discipline has been formed when it becomes closed and access to it requires specialist training. Moreover, as Hanne Andersen (in Wray) notes, in Kuhn's view 'analysis of communities as the "producers and validators of scientific knowledge" was directed not at entire disciplines such as physics or chemistry, nor at subdisciplines such as organic chemistry or solid-state physics, but at the narrow research field within which members communicate closely' (p. 201). Andersen goes on to note that scientific fields are much wider in our days and often require teams, not individuals, to investigate the questions of their discipline. However, the Kuhnian idea that research communities have 'perhaps one hundred members, occasionally significantly fewer' (p. 200), resonates with subfields of historiography of science. Again, the current state of historiography of science seems to align with Kuhn's ideas.

Kuhn or Thackray (or Sarton and other historical figures) cannot be ignored. I have argued that historiography of science, while ignoring Kuhn, itself satisfies Kuhn's view of what a mature discipline is like. How can the discipline ignore something the applicability of which is indicated by the discipline itself? Moreover, Thackray's views (as collected in Thackray) count as important by showing how a discipline is created by doing it and by creating and participating in meta-level discussions on the nature of the discipline. But again, this is what we expect from discipline building if we follow Kuhn's view.

Before the end, I *must* admit that I do not believe that Kuhn was correct about science and I do believe that the picture he provided was nothing more than a simplistic explanatory framework to be rejected once more details were figured out. However, one can reasonably look at these older sources and find serious ideas in them. Chang (in Wray) defends pluralism and complementary science where useful ideas lost in history are recovered and studied again to enrich our current understanding.[4] I think the same is true of the ideas in Kuhn and works such as Thackray's where views on science and the function of historiography of science are presented in a different manner than in today's discipline. Reading these two interesting and high-quality works provides a fruitful starting point.

Finally, I wish to raise a concern. We tend to think that science has changed so much that historiography of science cannot help us to understand it – even more, we tend to deem obsolete works that are several decades old and discuss periods of science hundreds

[3] Veli Virmajoki, review of Lukas M. Verburgt (ed.), *Debating Contemporary Approaches to the History of Science*, *BJHS* (2025) 58(1), pp. 167–8.

[4] See also Chang's other works, such as *Inventing Temperature: Measurement and Scientific Progress* (Oxford: Oxford University Press, 2004).

https://doi.org/10.1017/S0007087425101416 Published online by Cambridge University Press

of years ago. However, now we expect science to change quickly in coming years due to developments in AI and related technologies. In this context, the study of older ideas and their roots may not be a terrible idea. Even if the older ideas do not tell as such about recent science, they tell us how science changes and how it is related to human reality in general. And if it is the case, as many expect, that the 'recent science' will not any more be the science of tomorrow, understanding science as an activity embedded in changing human societies carries intrinsic value. To stay on track on what might be the science of the future, we need to look at science as it was in the past. Science will not be done in the same way, but we can remind ourselves how it is embedded in human societies nevertheless.

Cite this article: Virmajoki Veli, 'How to create a discipline? Looking back at luminaries', *The British Journal for the History of Science* 59 (2026), 115–119. https://doi.org/10.1017/S0007087425101416

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